



## Letter to the Editor

## Response to “Hemodynamic consequences of restraints in the prone position in excited delirium syndrome”



We thank Dr. Alain Michaud for his interest in our study. We apologize for the length of our response, but Dr. Michaud's letter is nearly the length of the original manuscript with as many references.<sup>1</sup> One could make the case that the letter might be appropriate as an original journal submission, but because the author relies on speculative theories supported predominantly by isolated case reports of questionable applicability and no true experimental literature, as well as the fact that the author mischaracterizes many of his cited references, it is highly doubtful that Dr. Michaud's letter would ever survive peer review.

To start, Dr. Michaud states “Continuous chest compression may be innocuous... or may result in cardiorespiratory arrest... as shown by Burke and Hare”, referring to the notorious 19th century pair who murdered very intoxicated individuals for the purpose of supplying anatomic specimens for the local medical school in Edinburgh, Scotland.<sup>2</sup> Dr. Michaud has fallen victim to the misperception that “burking” refers to an asphyxial death by chest compression. In actuality, Burke's own confession indicates quite the opposite and that victims were “burked” by suffocation, not by some form of compressional asphyxia. In his confession, Burke reports using pillows or hands to cover the nose and mouth of unconscious intoxicated *supine* victims to suffocate them and that “... they lay across the body at the same time... it was not done for the purpose of preventing the person from breathing, but was only done for the purpose of keeping down the person's arms and thighs, to prevent the person struggling.”<sup>3</sup> It is disappointing that Dr. Michaud did not review the historical record to more accurately portray the details of this case and furthermore it seems quite inappropriate to even compare an excited struggling individual with “superhuman strength” and a victim who is comatose from alcohol intoxication.

Dr. Michaud also mischaracterizes other references often leaving out key details in the experimental studies he uses to support speculative theories. For example, in discussing compressive chest forces, the author references a study of an experimental device that increased peak force over the chest by 680% over manual compression and “resulted in a right atrial peak pressure of 83 + 40 mmHg.”<sup>4</sup> Clearly the more pressure applied to the chest the greater the peak pressure. However Dr. Michaud does not mention that this device which applied pressure “over a large surface area of the chest” was in reality a constricting band that went around the *entire* chest and was specifically “programmed to provide a consistent 20% reduction in the anterior-posterior dimension of the chest during the compression phase”.

This circumferential constrictive band is entirely different than the downward vectors of force created by a knee in the back of a prone struggling subject and has no applicability to this type of scenario.

Dr. Michaud also states “pressure on the spine during surgery in the prone position may “markedly” obstruct right ventricular outflow tract (RVOT) in certain circumstances” citing a single case report as evidence. What Dr. Michaud fails to mention is that this case involved an adolescent with congenital scoliosis described as having marked narrowing of the anterior-posterior diameter of the chest.<sup>5</sup> More importantly however, is that although this patient did indeed have a significant decrease in RVOT diameter with weight applied and a reduction in flow in the prone position, this occurred *only* when the patient was positioned with transverse bolsters. When the patient was positioned prone with longitudinal bolsters, the authors “found appreciably less impact to the RVOT, RV size and flow, and CVP” *with and without* pressure to the back. While it is disappointing that Dr. Michaud quotes case reports as definitive science, this error is compounded by only giving “sound bites,” not the complete and accurate information.

Dr. Michaud makes a similar error with his poorly supported generalizations regarding CPR when he states “that the hemodynamical principles supporting chest compression during CPR are probably the same whether the individual is in the prone position or the supine position.” This rather sweeping pronouncement is based not upon any experimental evidence but rather a single case report of a 77-year-old woman under going surgery in the prone position who had a sudden unexpected blood loss of 3 L and developed severe bradycardia and hypotension for which CPR was initiated and her condition quickly improved with concomitant volume replacement.<sup>6</sup> From this, Dr. Michaud generalizes that CPR in the prone position is probably physiologically the same as in the supine position. This is quite a leap, based upon this single case of an elderly patient with a marked decrease in intravascular volume who never actually suffered cardiac arrest and likely never “required” CPR, rather just volume resuscitation. Once again, we are disappointed that Dr. Michaud relies on a case report as scientific fact for his opinions, but more importantly it seems quite strange to us to make a comparison between an excited struggling individual with “superhuman strength” to an elderly anesthetized individual who has just suffered an acute loss of blood volume.

In another synopsis of single case report, Dr Michaud implies that first responders may not be aware of the magnitude of the pressure applied to the chest which might precipitate an unexpected cardiac arrest in a prone position.<sup>7</sup> Without going into too great a detail, the author ignores the fact that the “250-lb

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attendant” was lying across the back and therefore there was no true estimate of the amount of weight actually being applied to the individual, but more importantly the “re-enactment” was terminated, not by any hemodynamic changes but rather because the individual playing the victim complained about the subjective inability to breathe. Once again it seems off the mark to confuse the respiratory or ventilatory effects of weight on the back with the cardiovascular effects, which is what our published study examined.

From this, Dr. Michaud also makes the very puzzling statement that “...1 knee to the ‘center of the back’ of individuals with ExDS should affect the venous return significantly.” No data studying this scenario are presented to support this conjecture, and it appears to be more speculation even though it is presented as an assumed logical fact. Moreover, Dr Michaud goes on to assert that this compression reduces the central blood volume by 50%, with once again, no data presented. As a group we find it quite amazing that a purported scientific theory is based upon such a “house of cards.”

Dr. Michaud also mischaracterizes the report by Stratton et al.<sup>8</sup> He states that “none of the individuals restrained in lateral position died” in this paper. Although technically correct, he fails to mention that the authors also state, “Although all sudden death victims were found prone, almost all of those who did not suffer death while restrained were also in the prone position.” Stratton then points out, “the data presented do not support or refute the prone position while hobble restrained as independently associated with sudden death in the setting of excited delirium.” Moreover, Stratton's work made no mention of weight applied to the back and thus there appears to be little relevance to our study and the central point of Dr. Michaud's letter.

Although we have more concerns and criticisms of the “interpretations” of additional references cited, we would like to point out the complete failure of Dr. Michaud to mention any of the experimental literature that has directly examined the physiological effects of weight applied to the back. In addition to our paper, Krauskopf et al. assessed the effect of weight on the back on the size of the IVC.<sup>9</sup> Although they demonstrated a reduction in the size of the IVC, which is not surprising since this is a compliant vessel, they also demonstrated no clinically significant adverse affects upon cardiac output. At least four other papers have examined different effects of weight applied to the back.<sup>10–13</sup> Although cardiac output was not directly measured in these papers there were no reports of any adverse effects such as blood pressure drops or reflex tachycardias, that would be consistent with marked reductions in cardiac output. In the paper by Cary, blood pressure and pulse actually increased with 165 pounds on the back in a prone position compared to the same position without weight. In the paper by Ho et al., with 147 pounds on the back, blood pressure and pulse were also increased. The results of both of these papers are inconsistent with the speculations found in Dr. Michaud's letter. Although the remaining two papers did not examine BP or pulse rate, in none of these were adverse hemodynamic effects reported in spite of a weight of 225 pounds being applied in one of the studies.<sup>11</sup> Thus Dr. Michaud completely fails to mention five other experimental papers that directly contradict his thesis for which he presents no directly applicable supportive experimental evidence. Finally, to be blunt, Dr. Michaud ignores common sense. Were as little as one knee in the back sufficient to cause such a dangerous reduction in cardiac output there would be significant death rates among rugby or football players.

There is one point with which we agree with Dr. Michaud. He states, “Considering the prevalence of coronary heart disease and left ventricular hypertrophy in fatal ExDS a sudden decrease in myocardial oxygen delivery would be detrimental.” This is of course

obviously true, however it surprises us that Dr. Michaud appears to ignore the role structural heart disease likely plays in sudden cardiac death. There is now abundant evidence that even relatively minor degrees of LVH is associated with an increased incidence of sudden death and it seems extraordinary to us that this risk factor for death is relegated to relative unimportance in favor of an unfounded and unsupported theory.<sup>14,15</sup>

The only legitimate question specific to our published study raised by Dr. Michaud is whether the 20 × 20 cm cutout may have prevented compression of the heart chambers and IVC. Since this speculative criticism would require the chest wall to actually protrude through that opening we consider this to be highly unlikely as this was not observed echocardiographically. Additionally, our data did show that the IVC was reduced in size, consistent with the findings of other researchers, including Krauskopf and Ho. It is unlikely our cutout would have prevented compression of the heart chambers with weight force while simultaneously demonstrating actual compression of the IVC.

Lastly, Dr. Michaud did cite O'Halloran's paper on restraint asphyxia published in 2000 where some of the very same ideas expressed in this letter were promulgated. In a published correspondence following that publication Drs. Gulino and Young suggested, “the concept of restraint asphyxia as presented in this paper and others does not have a scientific basis. We believe the concept should be disappear like the dinosaur.”<sup>16,17</sup> We could not agree with them more.

*Conflict of interest*  
None.

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